

Das Karstphänomen. Versuch einer morphologischen monographie von Dr. Jovan Cvijić. (Wien: Ed. Hölzel.)

NOTWITHSTANDING its rather pompous second title, this is an interesting and valuable book, which, however, is not a separate work, but the third part of the fifth volume of a geographical series (*Abhandlungen*) edited by Prof. A. Penck. Its subject may be briefly stated as follows:—In many limestone districts the surface of the rock is guttered by channels—sometimes small, sometimes large—varying from comparatively smooth to rough. Here each ends in a small pipe, which descends vertically into the rock; there they converge towards one of larger size. With this system of superficial drainage are associated hollows of various forms, “blind valleys,” and the like, and caves are likely to be common. A region which exhibits some or all of these phenomena is called, from the peculiar sculpture of the surface, a *karst* region. Such may be found in various parts of the world. It is represented in England by the furrowed limestones and “swallow-holes” of Derbyshire and Yorkshire; it occurs in many parts of the Alps, the phenomena becoming more frequent eastward, till their headquarters are reached in the Julian Alps and the great “Karst plateau,” north of the Gulf of Fiume. As they occur in many lands, so they bear many names. A full, exhaustive, and elaborate account of these interesting phenomena will be found in this memoir, perhaps with an affected attempt at precision in distinction and classification (for after all, though curious, they are simple in origin), together with abundant references to the literature of the subject. Its usefulness, however, would be greatly increased by an index or by a very full table of contents; and though it is paged continuously with the volume, the latter, at least, ought to have been given.

T. G. B.

LETTERS TO THE EDITOR.

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The Origin of Lake Basins.

THE most thorough-going glacialist could find no ground for complaint that Dr. Wallace has not gone far enough in his most interesting advocacy of the glacial origin of lakes. I do not propose to enter into any general discussion of this question; that glaciers can excavate rock basins is indisputable, but there is a limit to their power, and this limit I believe to be reached far short of even the larger of our English lakes. The controversy is of long standing, and there is little new to be said on either side; nor would I have desired to re-enter it, but that Dr. Wallace's article seems to me to contain one serious fallacy and one vital misstatement which have not as yet been noticed, though they should not be left uncorrected.

The fallacy is not a new one; it may be found in the writings of more than one of the advocates of the glacial theory, and is contained in the argument that because lakes are found in regions that have been extensively glaciated, and are not found in regions precisely similar in every respect, except that there has been no great extension of glaciers, therefore the rock basins in which the lakes lie were excavated by glaciers. I trust I have not misrepresented the argument in this succinct statement of it; but such condensation is useful if we would detect a fallacy, and in this condensed form the fallacy of the undistributed middle term becomes conspicuous. The term “lake” is by no means coextensive with the term “rock basin,” and it is not the water filling the lake which requires explanation so much as the basin that it fills. A rock basin filled with alluvium is a rock basin still, and requires explanation as much as if it contained water, and was consequently a lake.

The misrepresentation is to be found in Dr. Wallace's limitation of what he rightly regards as the only tenable alternative theory, that the rock basins owe their origin to deformation of the surface immediately before the advance of the ice. This

limitation of time is so extraordinary that it would have passed for an accident or oversight, but that it is repeated at greater length on the very next page; did it form any essential part of the theory, this would deserve all the strictures passed upon it, but such is by no means the case. Without entering into the question of whether the geologists quoted by Dr. Wallace accept this limitation of time, I may point out that it is altogether more reasonable to regard the deformation as having taken place *after* the advance of the glaciers. We know that during the glacial period there were great changes of level, and it is reasonable to suppose that these were not absolutely uniform; moreover, had the rock basins been formed before the ice was there to fill them, they would mostly have been filled at once by river deposits, as has been the case in nonglaciated regions, and once filled up they would remain so on this theory, for if a glacier cannot erode a rock basin it cannot clean out one that has been filled up with stream deposits. This alteration of time makes the theory more natural and acceptable; when a rock basin is formed in the course of a stream by elevation or subsidence no lake arises in the great majority of cases, as either the barrier is destroyed by corrosion, or the hollow is filled up by deposition, as fast as it is formed; but when the basin arises underneath a glacier it becomes occupied by ice, and on the retreat and disappearance of the glacier a hollow is left that is at first filled by water, forming a lake, and only subsequently by degrees filled up by stream deposits. In this way the connection between the present distribution of lakes and the areas of pleistocene glaciation is easily explicable, and it is consequently not admissible as an argument to prove that the lake basins were excavated by glaciers until it is shown that in the nonglaciated regions, where there are now no lakes, there are also no rock basins.

With most of the regions quoted by Dr. Wallace I have no personal acquaintance, but in India such do certainly occur, and have as certainly not originated by glacial erosion; in some cases the existence of the rock basin has been proved by borings, but besides these there are many more instances where there can be no reasonable doubt of the existence of a rock basin, though the final test has not been made.

R. D. OLDHAM.

IN his last communication Sir Henry Howorth makes two statements which are so erroneous and so misleading that I cannot allow them to pass without correction. The first is, that Mr. Deeley “repudiates Dr. Wallace's notion that regelation can in some way act as a compensating element when crushing supervenes in ice.” Here is a double misstatement. Mr. Deeley “repudiated” no notion of mine, or he would, I am sure, have said so plainly, and he said nothing whatever about “crushing.” Neither did I say a word about regelation acting as a “compensating element,” for I do not believe in the crushing of glaciers by their own pressure. I asked Sir Henry what would happen to the ice after it was crushed, the pressure continuing; and I get no reply but the above double misstatement.

Then, further on, Sir Henry says: “Mr. Wallace confesses he does not like to face these mechanical issues.” This is simply untrue. I “confessed” nothing of the kind, and I challenge Sir Henry Howorth to quote any words of mine which will bear such a meaning. I maintain that his “mechanical issues” are pure theories, and are beside the question of the actual facts of glacier notion. Lastly, he attempts to evade the real issue between us, which is, that he himself accepted Charpentier's conclusions as to the extent of the Rhone glacier, but refuses to allow me to use these same conclusions as a datum in the discussion.

I have now shown ample reason why further discussion of this matter with Sir Henry Howorth must be unprofitable.

ALFRED R. WALLACE.

The Second Law of Thermodynamics.

I AM unable to see any reason for regarding Clausius' supposed deduction of the Second Law as in any way limited by the condition stated by Mr. Burbury, viz. “that the system be conservative, that is, that the external as well as the internal forces acting on it are to be derived from a potential.” No such limitation was contemplated by me when I was preparing the Report for the British Association.

It is true that this assumption is made in § 17 of the Report, in order to establish the closest possible connection between the

deduction and Hamilton's Principle of Least Action. But the limitation is required by the postulate, viz. the Principle of Least Action, not by the deduction. Clausius expressly states (*Phil. Mag.* vol. xlv. (1872) p. 365) that his deduction holds good for systems to which the Principle of Least Action is not directly applicable, and in consequence he claims that his equations involve a new principle which is of more general application than Hamilton's Principle (*vide* Report, § 16). I think there can be little doubt that Clausius had in his mind the very objection which Mr. Burbury now raises, and that it was in order to meet it that he claimed this generalisation.

The assumption as to the conservative nature of the forces is not required except in § 17 of the Report, and at the end of that paragraph two methods of avoiding it are suggested. One is to assume that the force-function can be varied with the time, the other is a method adopted by Von Helmholtz.

If we allow the force-function to be varied with the time, then in Mr. Burbury's case (of a column of gas held down by a piston of constant mass) the potential of *gravity* can be altered and therefore the *weight* of the piston is disposable. This disposes of Mr. Burbury's objection, and it only remains to consider the investigations given at the beginning of § 17 of the Report.

Clearly the Second Law of Thermodynamics cannot be deduced from studying the behaviour of gas under constant pressure. To establish it we must make the working substance undergo a reversible cycle in which heat is absorbed and external work performed. To do this we attach the piston to a crank as in an ordinary steam engine, and make it turn a wheel which raises a weight by means of a windlass. Here we have a strictly conservative system, and one to which the arguments of § 17 are therefore strictly applicable. And for every single turn of the wheel we have

$$\int \frac{dQ}{T} = 0 \quad \dots \dots \dots (I)$$

a relation identical in form with that which expresses the Second Law.

It seems to me that the real objections to Clausius' deductions are far more intricate and far less easily disposed of. The difficulty of assigning a physical meaning to the quasi-period $\bar{t}$ is one of them, and there are other difficulties connected with the interpretation of $\bar{T}$ as absolute temperature when intermolecular forces are taken into account. All these difficulties are alluded to in my Report, and they are not peculiar to the hypothesis of "quasi-periodic" motions; similar difficulties exist in some form or other in most so-called "proofs of the Second Law."

It may be interesting to mention that a proof of the Second Law based on the virial equation

$$p\bar{v} = \frac{3}{2}(T + \Sigma \Sigma (\frac{1}{2}r^2))$$

was given by R. C. Nichols in the *Philosophical Magazine* for 1876 (v. Series I. p. 369). I hope later on to deal more fully with that portion of Mr. Burbury's letter which relates to the "virial proof."

G. H. BRYAN.

December 21.

Flame.

I HAD hoped that after disavowing the unpleasant interpretation which had been put upon his first letter, Dr. Armstrong would have done me the honour, and himself the justice, of indicating precisely where he disagreed with my scientific arguments. Instead of doing so, he has imputed to me a sensitiveness to criticism so excessive that he feels it best to retire from the controversy with a mere statement that our standpoints are different. I must leave it to the readers of NATURE to judge whether Dr. Armstrong has any longer the right to claim a *standpoint*.

Mr. Newth will find a fuller discussion of my views about flame in the *Journal of the Chemical Society* for 1892, pp. 204-226. If after reading that he still has difficulty in understanding the fundamental points of my work, I shall be glad to help him if he will communicate with me privately. With a little care, Mr. Newth will find it quite easy to separate the two cones of a carbon monoxide-air flame in the ordinary apparatus without the use of a gauze cap. The air must be turned on very gradually. In the case of the hydrogen-air flame it is best to dilute the gases with nitrogen, as recommended in my first paper.

I am sorry that anyone should think I have slighted Dr. Frankland's work. I can, however, understand, and even admire, Mr. Newth's excessive zeal in the matter.

ARTHUR SMITHELLS.

"The Zoological Record."

WE are delighted to find such a consensus of opinion as to the desirability of retaining palæozoology in the *Zoological Record*. The recorders whom we have consulted, the editor, and now the secretary of the Zoological Society, all have expressed themselves in its favour. The question therefore is purely one of finance. Under these circumstances the publication of the correspondence with the Geological Society is of great interest, and the only addition that we could suggest would be the publication in your columns of that poverty-stricken society's balance-sheet.

We should like, however, to point out that the *Zoological Record* appeals to the Geological Society, not merely on the ground of its palæontological contents. Palæontologists go to the *Record* to learn what the neontologists are doing, quite as much as to read the titles of their own papers. Under any circumstances, then, the *Zoological Record* has some claim on the Geological Society, and we must all regret that financial distress prevents the Society from acknowledging that claim.

But the Record Committee of the Zoological Society need not despair; for the *Record* has no less claims on many other of our learned societies, and, by the converse argument, the inclusion of palæozoology merely strengthens those claims. Every biologist should be grateful to those who bring to his notice literature that he would otherwise never hear of. Apart from this, one-third of the volume is devoted to entomology. Why should the Entomological Society not be invited to contribute? Then there are the Royal and the Linnean Societies, and the British Association; the Microscopical, we would mention, did it not already do excellent work of a similar kind. At any rate, surely five of these bodies could be prevailed on to subscribe £20, or even £40 a-piece. The Zoological Society appeals through its *Record* to hundreds of workers who do not belong to it. It has long done an admirable work, of which it will never lose the credit. Everyone should sympathise with it in its present attempt to perfect this work, and should not permit it to suffer so large a pecuniary loss in that attempt.

December 17.

R. I. POCCOCK.

F. A. BATHER.

On the Bugonia-Superstition of the Ancients.

LAST August, I published in the *Bulletin Soc. Entomol. Italiana*, 1893, p. 186-217, an article entitled "On the Bugonia of the Ancients, and its relation to *Eristalis tenax*, a two-winged insect." I desire to collect some more materials on that subject, in view of a second edition, and I would be very grateful to readers of NATURE who may be able to give me assistance in that matter.

The information I require may be expressed in two questions: (1) Whether travellers in out-of-the-way places in Europe or Asia have not come across vestiges of the superstition about oxen-born bees, still lingering among primitive people?

(2) Whether readers of Oriental literature have not come across passages evidently referring to this superstition, like the passage I reproduce here as an example. I found it in the "Golden Meadows" of the Arab traveller Massoudi (died in Cairo, 955), translated by Barbier de Meynard and Pavet de Courteille, Paris, 1861, vol. iii. p. 233. It relates a conversation which took place in Arabia, and of which this is a fragment. "Had the bees, which produced this honey, deposited it in the body of a large animal," asked Viad? The surveyor answered: "Hearing that there was a hive near the sea-coast, I sent people to gather the honey. They told me that they found at that place a heap of bones, more or less rotten, in the cavity of which bees had deposited the honey that they brought with them."

I have sent separate copies of my paper to the Geographical, Linnean, and Entomological Societies in London, to the Natural History Museum, South Kensington, to the Athenæum Club, and to many friends in England. I should be happy to send a copy to anybody interested in the subject.

C. R. OSTEN SACKEN.

The Earliest Mention of the Kangaroo in Literature.

I TAKE advantage of the present opportunity to put another question to zoologists. In the same book of Massoudi, whom I quoted in the previous notice, I found the following passage (vol. i. p. 387):—"El Djahiz, in his 'Book on Animals,' relates that the female rhinoceros is pregnant for seven years,